

ELEMENTS AND COMPOUNDS.<sup>1</sup>

I HAVE the honour of speaking to an audience of many men whom I have long venerated as my intellectual, although not my personal, teachers, and whom I admire as leaders in our common work for science. But however admirable the *present*, I am still more impressed by the thought of the *past* associated with this place. When, not long ago, I was engaged in electrochemical investigations and almost daily sought for information and enlightenment in Faraday's researches, I did not dare to think in my boldest dreams that one day I should find myself standing on the very spot in which he was wont to give the first accounts of the innumerable results of his indefatigable labours, his indomitable zeal, and his inexorable love of truth.

All that the pupil can do in such a case is to imbue himself as completely as he can with the ideas of the master and to try to perform his modest work in the master's spirit. But here arises a new difficulty: what subject ought I to choose? When I look into my own humble efforts, I find everywhere traces of Faraday. So far as relates to electrochemistry, the thing is plain; I think there is no word that I have oftener spoken or written than the word "ion," that word which was uttered for the first time in its modern sense in this very spot. But in other fields in which I have also worked, I feel the influence of his skilful hands and his keen vision. Catalysis, which I have studied during the past ten years, likewise came under his hands; and in the parts of the subject he worked at, the charm of secrecy and inexplicableness has been exchanged for the better qualities of a problem capable of resolution by earnest workers. And in one subject which has engrossed a very great part of my scientific activity, in the question of energy, I find the venerated master again a leader. He was indeed the first scientific man to direct all his investigations in view of the idea of the conservation and the mutual transformation of the various forces, as he called them, or the various kinds of energy, as we call them now.

This is a side of Faraday's mind to which, perhaps, not so much attention has been paid as it deserves. Although doubtless the greatest advance—the discovery of the quantitative proportionality between the energy which disappears and that which originates—was due to Mayer and Joule at a later date, yet the practical perception of this relation was working in Faraday's mind long before. There is indeed a great difference between the intellectual development of a scientific truth to a degree sufficient for the discoverer's *own* work, and to the degree required for its successful transfer to the minds of *other* workers. Faraday contented himself in this case, as well as in others (for example, in his conception of lines of force), with the first step. But that he had reached this step and stood firmly on it, that he used this conception constantly and regularly in his work, is evident from his constant reference to it from the first year of his scientific work onwards. From a closer study of his lectures and papers we learn that in every case he put the question: how can I change a given force into another? This continued to the very end of his work; for the last experiments he made related to the direct conversion of gravity into electricity, and although he did not succeed in his attempt, he was nevertheless convinced of the possibility of the conversion.

Guided by these considerations, I directed my attention to the very earliest problems treated by the master. Even before Faraday held the chair of chemistry here in the Royal Institution, as a youth of twenty-five years of age he practised the art of a lecturer in a small club, the City Philosophical Society, and the first course which he delivered there was on chemistry. In the sixteenth lecture, after a description of the metals, he concluded with the following general remarks:—

"To decompose the metals, then, to reform them, to change them from one to another, and to realise the once absurd notion of transmutation, are the problems now given to the chemist for solution. Let none start at the difficult task and think the means far beyond him; everything may be gained by energy and perseverance." And after a description of how in the course of history the means necessary

for the isolation of the metals from their combinations have grown ever more and more efficacious, he mentioned the recent great discoveries of his master Davy as follows:—

"Lastly, glance but at the new, the extraordinary powers which the chemist of our own nation put in action so successfully for the reduction of the alkalies and the earths, and you will no longer doubt that powers still more progressive and advanced may exist and put at some favourable moment the bases of the metals in our hands."

When I try to follow this hint and take for the object of our consideration the question of the nature of the elements and of their compounds, I am aware that I am not the first who has done so in this place. If I am not mistaken, the very first chemist who had the honour of addressing you as a Faraday lecturer, Jean-Baptiste Dumas, lectured thirty-five years ago on the same subject. Nevertheless, I do not shrink from the repetition. Every generation of chemists must form its own views regarding this fundamental problem of our science. The progress of science shows itself in the way in which this is done. Faraday was at this time fully influenced by Humphry Davy's brilliant discoveries, and sought for the solution of the problem in Davy's way. For Dumas, the most important achievement of the science of his day was the systematising of organic chemistry, condensed into the concept of *homologous series*. He therefore regarded the elements as comparable with the hydrocarbon radicles, and tried to arrange them in similar series with constant differences in the numerical values of their atomic weights. It is well known that these ideas finally developed into the great generalisation we owe to Newlands, Lothar Meyer, and Mendeléeff. Although the problem of the decomposition of the elements was not solved in this way, these ideas proved to be most efficient factors in the general development of science.

From what store of ideas will a modern chemist derive the new materials for a new answer to the old question? A physicist will have a ready answer: he will construct the elements in a *mechanical* way, or, if he is of the most modern type, he will use *electricity* as timber. The chemist will look on these structures with due respect indeed, but with some reserve. Long experience has convinced chemists (or at least some of them) that every hypothesis taken from another science ultimately proves insufficient. They are adapted to express certain sides of his, the chemist's, facts, but on other not less important sides they fail, and the end is inadequacy. Learning by this experience, he makes a rule to use only chemical material for this work, and according to this rule I propose to proceed.

Hence, like Dumas, I put the question: what are the most important achievements of the chemistry of our day? I do not hesitate to answer: *chemical dynamics* or the theory of the progress of chemical reactions and the theory of chemical equilibrium. What answer can chemical dynamics give to the old question about the nature of the chemical elements?

The answer to this question sounds most remarkable; and to impress you with the importance I ascribe to this investigation, I will mention the result at once: *It is possible, to deduce from the principles of chemical dynamics all the stoichiometrical laws; the law of constant proportions, the law of multiple proportions and the law of combining weights.* You all know that up to the present time it has only been possible to deduce these laws by help of the atomic hypothesis. Chemical dynamics has, therefore, made the atomic hypothesis unnecessary for this purpose, and has put the theory of the stoichiometrical laws on more secure ground than that furnished by a mere hypothesis.

I am quite aware that in making this assertion I am stepping on somewhat volcanic ground. I may be permitted to guess that among this audience there are only very few who would not at once answer, that they are quite satisfied with the atoms as they are, and that they do not in the least want to change them for any other conception. Moreover, I know that this very country is the birthplace of the atomic hypothesis in its modern form, and that only a short time ago the celebration of the centenary of the atomic hypothesis has reminded you of the enormous advance which science has made in this field during the last hundred years. Therefore I have to make a great claim on your scientific receptivity. But still I do not hesitate one moment to lay the results of my work before you. For I feel quite sure

<sup>1</sup> By Prof. W. Ostwald. Faraday Lecture delivered before the Fellows of the Chemical Society in the Theatre of the Royal Institution on April 19.

that I shall find this receptivity unrestricted; and, moreover, I shall reap another advantage. For I also feel assured that you will offer me the severest criticism which I shall be able to find anywhere. If my ideas should prove worthless, they will be put on the shelf here more quickly than anywhere else, before they can do harm. If, on the contrary, they should contain anything sound, they will be freed here in the most efficacious way from their inexact and inconsistent components, so as to take the shape fittest for lasting use in science. And now let us go into the matter.

The first concept we start from is *equilibrium*. In its original meaning, this word expresses the state of a balance when two loads are of the *same weight*. Later, the conception was transferred to forces of all kinds, and designates the state when the forces neutralise one another in such a way that *no motion* occurs. As the result of the so-called chemical forces does not show itself as a motion, the use of the word has to be extended still further to mean that *no variation* occurs in the properties of the system. In its most general sense, *equilibrium denotes a state independent of time*.

For the existence of such a state it is above all necessary that temperature and pressure shall remain constant; in consequence of this, volume and entropy remain constant too. Now it is a most general experimental law, that the possibility of such a state, independent of time, is dependent on the *homogeneity* of the system. In non-homogeneous bodies, as, for instance, in a solution of different concentration in different places, or in a gaseous mixture of different composition in different places, equilibrium cannot exist, and the system will change spontaneously into a homogeneous state. We can therefore limit our considerations to this state, and we shall consider only bodies or systems of bodies in equilibrium, and, consequently, homogeneous.

Perhaps the possibility of the existence of water in contact with water-vapour might be considered contradictory to this statement, because we have here two different states and no homogeneity. Here we meet with the new concept created by Willard Gibbs, namely, that of a *phase*.

Systems of this kind are formed of homogeneous bodies indeed, but of more than one. The water in our system is homogeneous in itself, and the vapour too, and equilibrium cannot exist until both are homogeneous. But there is a possibility that a finite number of different homogeneous bodies can exist together without disturbing one another. In such a system we must have the same temperature and the same pressure everywhere, but the specific volume and the specific entropy may change from one body to the other.

We call a *phase* every part of the system where these specific properties exhibit the same value. It is not necessary that a phase should be connected to one body only; it may be distributed over any number of parts. In this way the millions of globules of butter in milk form only *one* phase, and the watery solution of casein and milk-sugar forms a second phase: milk is a two-phase system.

*Every system consisting of only one phase has two degrees of freedom.* This law involves only the assumption that the sole forms of energy involved in the system are heat and volume-energy; we exclude from consideration any effects due to gravitation, electricity, surface-tension, &c. This law is connected with the famous phase rule of Willard Gibbs, but is not identical with it, for it contains no mention at all of the so-called components of the system. Indeed, the law is valid in the same way for any pure chemical element, for example, oxygen, or for any mixture, for example, a glass of whisky and water. If you allow to the latter only one phase, it is impossible to change it in more than two ways, namely, in pressure and temperature.

The existence of such a body in the shape of only one phase is generally limited. If the pressure be lowered at constant temperature, a liquid or a solid will change at last into a gas. Lowering of temperature will change a

gas into a liquid and a liquid into a solid. For every one-phase system it is possible to determine a "sphere of existence." This sphere is not necessarily limited on all sides; for gases we do not expect a limit on the side of low pressures and high temperatures, nor for solids on the side of high pressures and low temperatures. But on certain sides every phase has its limits, and most of these limits are experimentally accessible.

What will happen if we exceed the limit of existence of a phase? The answer is most simple: *a new phase will be formed*. The spheres of existence of the different phases therefore limit one another, and the boundary-lines represent the interdependent values of temperature and pressure for the possibility of the co-existence of both phases.

By granting the co-existence of two phases we lose therefore one degree of freedom. At the same time a new variation has arisen from the ratio between the masses of the two phases. For we must not suppose that this ratio is without influence on the state; indeed we find here two radically different cases.

The most general case is, that during the transformation of one phase into another the properties of both are continually changing, and the state of every phase is therefore dependent on the ratio of the two masses. By evaporating sea-water at constant temperature the density of the residue grows continually higher, while the pressure, and therefore the density, of the vapour goes on decreasing. If, however, we evaporate distilled water, we do not find any change in

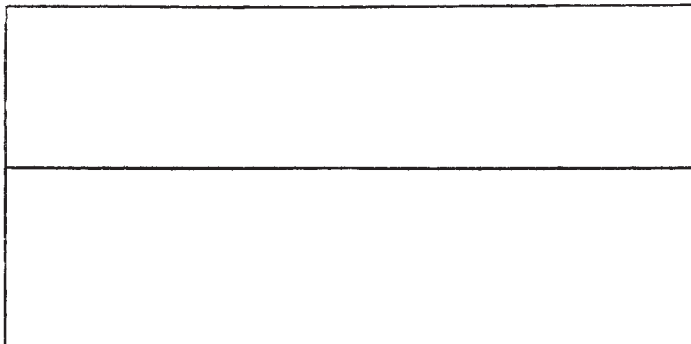


FIG. 1.

the properties of the residue and of the vapour during the whole transmutation.

Bodies of the first description we will call *solutions*, and of the second, *hylotropic bodies*. You will be inclined to call the latter substances or chemical individuals, and indeed both concepts are most nearly related. However, the concept of a hylotropic body is somewhat broader than that of a substance. But the possibility of being changed from one phase into another without variation of the properties of the residue and of the new phase is indeed the most characteristic property of a substance or chemical individual, and all our methods of testing the purity of a substance or of preparing a pure one can be reduced to this one property; anyone may readily convince himself of this by investigating any such method in the light of this description.

If we represent these cases by means of rectangular co-ordinates, taking as abscissæ the part of the first phase converted into the second, and as ordinates pressure or temperature, we get Fig. 1 for hylotropic bodies; they are represented by a horizontal straight line. With a solution we get a continuous line too, but not horizontal and generally not straight. If the ordinates are pressures at constant temperature, and the change is from liquid into vapour, the line will slope downwards as Fig. 2 shows. At other temperatures the lines will be of similar shape, only lying higher at higher temperatures and *vice versa*. With other changes we obtain similar lines, sloping upwards or downwards as the case may be. For simplicity's sake we will consider in the future only vaporisation; this case gives the greatest possible variety, and we are sure not to omit anything by such a limitation.



What is the general process of change in a solution while it is being vaporised? The answer is quite distinct: *the residue is always less volatile than the original solution, and the distillate more volatile*. If there were an example of a solution behaving in the contrary way, then the process of vaporisation at constant temperature would be an explosive one. For the vapour begins to form at a given pressure; if by this the vapour-pressure of the residue were lowered, the vaporisation would continue of itself at a continually accelerated rate until all the liquid would be vaporised at once. It would be, in other words, a *labile*

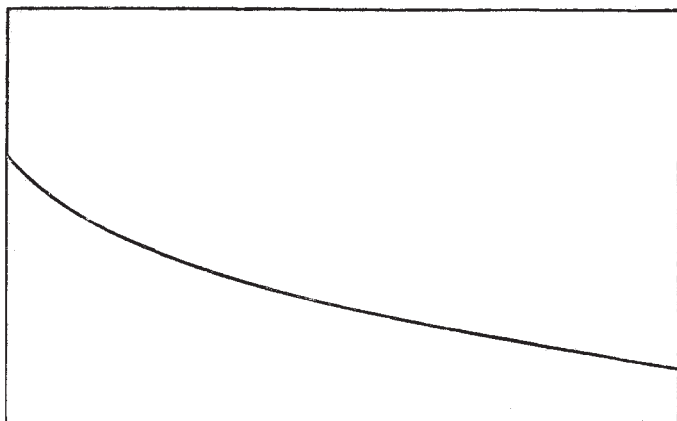


Fig. 2.

equilibrium. These equilibria are, however, only mathematical fictions, and have no experimental existence. If, on the contrary, the residue has a lower vapour-pressure, then the process is self-limiting, and shows the characteristics of a *stable* equilibrium. With hylotropic bodies we have an *indifferent* equilibrium, because the state is independent of the progress of the transmutation.

This being granted, we can ask: if we continue the separation of a solution into a less and a more volatile part by repeated distillation, what will finally become of it? Generally considered, two cases may happen. First the residue may become less and less, and the distillate more and more volatile, and there is no end to the progress. This case we may exclude from experimental evidence of a most general character, for we may take it as a general law that it is impossible to enhance any property beyond all limits, even by the unlimited application of our methods. We must conclude, therefore, that we shall ultimately meet with a *limit of volatility on both sides*, that finally we shall have separated our solution into a least and a most volatile part, and that both parts will not change further by repeated distillation. This is a most interesting result, for it means that *every solution can be resolved into components, which are hylotropic bodies*. For simplicity's sake we have considered only the case that two hylotropic components are generated by the process of separation; generally more than two may be formed, but in every case only a limited number of such components is possible. We may formulate therefore as a general law:—

*It is possible in every case, to separate solutions into a finite number of hylotropic bodies.*

From the components, we can compose the solution again with its former properties. This is also a general experimental law; if exceptions seem to exist, it is only because the case is not one of true equilibrium. Still we may limit our consideration to those cases where the law holds good. Then we have a relation between the properties of any *solution*, and the nature and relative quantity of its hylotropic components, which admits of only one interpretation.

Every solution of distinct properties has also a distinct composition and *vice versa*.

If we consider for simplicity's sake solutions of only *two* components, we may represent any property as depending upon the composition in a rectangular coordinate system, the abscissæ giving the composition and the ordinates the value of the property considered. In this way, we get a continuous line of a shape dependent on the particular case chosen.

If we consider the boiling points of all solutions formed by two hylotropic components, the most simple forms of curves (indeed the only experimental ones known) are given by the types I, II, and III, Fig. 3. For any solution, for example, the solution with the abscissa *a*, we can foretell its variation on distillation by the slope of the curve. For, as the residue must be less volatile, the residue will change to the ascending side of the curve. This is for I and III to the right, for II to the left side of the diagram. The change of the *distillate* is the opposite.

If we try to apply this criterion to the points *m* of the curve II and III, where there is a maximum and a minimum of the boiling point, we arrive at no decisive answer, for if the boiling point is already the highest possible it cannot rise, and if it is the lowest possible it cannot fall. We are forced therefore to conclude that the boiling point cannot change at all, that is, that this special solution must behave as a hylotropic body.

This is a well known theorem of Gibbs and Kononoff, to wit, that a maximum or a minimum, generally spoken of as a *distinguishing point* in the boiling-curve, is necessarily connected with the property of distilling without change in the composition of the solution. A similar law holds good for the transitions from liquid to solid and from solid to gas.

Now this looks like a contradiction; while a few minutes ago we placed solutions in a class exclusive of hylotropic bodies, we have here solutions, that is, mixtures, which behave like hylotropic substances. But the contradiction vanishes if we consider a series of boiling-point curves corresponding to various pressures. We then find that the composition at the distinguishing point does not remain

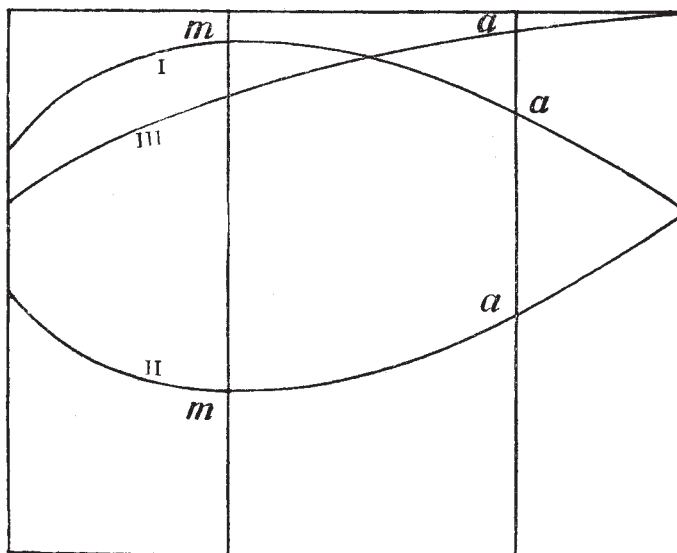


Fig. 3.

constant under different pressures, but shifts to one side, with alteration of pressure. This fundamental fact was discovered and experimentally developed in an admirable way by Sir Henry Roscoe, and has since proved itself a most important criterion in recognising a chemical individual.

By drawing curves corresponding to various pressures, we get therefore generally the diagram shown in Fig. 4, the loci of the distinguishing points forming one curve. Between

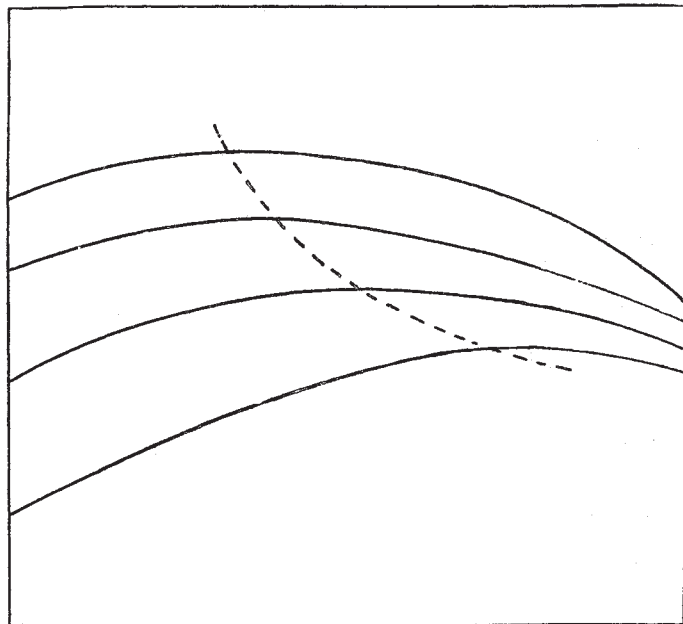


FIG. 4.

the infinite possibilities of the shape of this curve we have a distinguishing case again, the case that the curve is a vertical straight line. This means that the composition is independent of the pressure. When this is the case, we call this hylotropic body a substance or a chemical individual.

Therefore we conclude that a connection exists between solutions and chemical compounds or substances, the latter being a distinguishing case of the former. On the other hand, we get an exact definition: a substance or a chemical individual is a body, which can form hylotropic phases within a finite range of temperature and pressure.

Such substances can often be produced from other substances in the same way as a solution is, namely, by putting them together. If that can be done, we may infer from our definition that there exists a definite ratio between the components, independent of temperature and pressure between certain limits.

Now, this is essentially the law of definite proportions, the first of the stoichiometrical laws. We have deduced, therefore, the law of constant proportions from the concept of the chemical individual.

As you have seen, this deduction is extremely simple; the constancy of composition is a natural consequence of the mode of preparation and purification of chemical substances.

If we exceed the limits of temperature and pressure, where the body behaves as a hylotropic one, it assumes the properties of a solution, that is; its distinguishing point begins shifting in composition when the temperature is changed. Then it becomes possible to separate the body into its components, and we call this state the state of dissociation of the substance in question. In our graphic representation, the hitherto straight vertical line of distinguishing points turns sideways, Fig. 5. Most substances behave in this way, but there are substances which have never been transformed into solutions or the sphere of existence of which covers all accessible

states of temperature and pressure. Such substances we call elements. In other words, *elements are substances which never form other than hylotropic phases.*

From this we may conclude that every body is finally transformable into elements, and into only one definite set of elements. For the most general case is a solution. Every solution can be separated into a finite number of hylotropic components, and these again can generally be transferred into a state when they behave like solutions and can be separated further. Finally, the components remain hylotropic through the whole range of temperature and pressure, that is, they are elements.

From the fact that the relation between a compound substance and its elements admits of only one qualitative and quantitative interpretation, we derive the conclusion that the resolution of any substance into its elements must always lead to the same elements in the same proportion. Here we find the source of the law of the conservation of the elements. This law is not generally expressed as a special stoichiometrical law, because we tacitly infer it from the atomic hypothesis. But it is truly an empirical law, and we see that it is not only a consequence of the atomic hypothesis, but also a consequence of the experimental definition of an element and of our methods of obtaining elements.

Here I should like to pause for a moment for the purpose of quoting a couple of historical facts. Up to the present moment, the question whether it is possible to deduce the stoichiometrical laws without the help of the atomic hypothesis has only been raised by other investigators in order to deny the possibility.

So far as I am aware, there exists only one man who has worked upon the question with the earnest hope of obtaining an affirmative answer. Very few

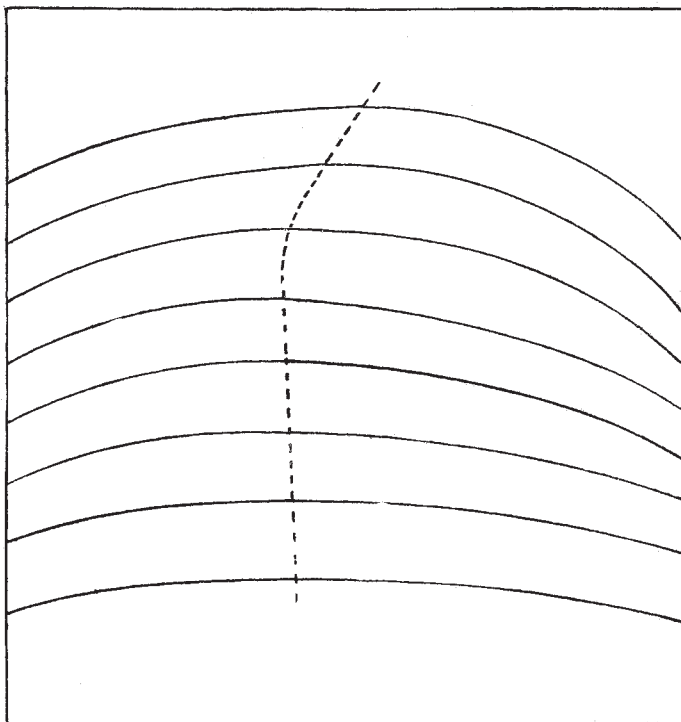


FIG. 5.

know his name. The man is Franz Wald; he is chief chemist at the iron works in Kladno, Bohemia. His papers on the subject are to be found in the *Zeitschrift*

für physikalische Chemie and in the *Annalen der Naturphilosophie*.

In the foregoing considerations, Franz Wald has played a great part. To him I owe first the idea that the definition of substances and elements is in a certain sense arbitrary, though very helpful and convenient. This definition is a condensed expression of our methods of separating and purifying these bodies. While, generally speaking, every solution has the same claim to be investigated as these bodies, the latter soon distinguish themselves as standards to which all other cases may be referred. To Franz Wald I owe further the idea that the conception of a *phase* is a far more general one than that of a substance, and that the deduction of the idea of a substance, and, further, the deduction of the laws governing the nature of substances, must start from the conception of the phase. I do not know whether Wald will agree with the way I have manipulated his ideas, but I feel it imperatively necessary to express my deep respect for, and my thankful obligation to, this solitary philosopher, who has prosecuted his work during a long series of years almost wholly without encouragement or sympathy from others.

Now there are still two stoichiometrical laws to be deduced, namely, the law of multiple proportions and the law of combining weights. I prefer to invert the order, and first to deduce the second law. It expresses the fact that it is possible to ascribe to each element a certain relative weight in such a way that every combination between the elements can be expressed by these weights or their multiples.

We suppose three elements, A, B, and C, given, which may form binary combinations, AB, BC, and AC, and besides these a ternary combination, ABC; there shall be but one combination of every kind. Now we begin by forming the combination AB; for this purpose, we must take a certain invariable ratio between the weights of A and B, according to the already proved law of constant proportions. Now we combine AB with C and get the ternary compound, ABC. There will be a certain ratio, too, between AB and C, and we can, if we put A as unity, assign to B and C certain numbers describing their combining weights relatively to A.

Now we begin to combine A with C forming AC, and then we form the ternary combination, ACB from AC and B. According to our law of a relation between elements and compounds, which can be interpreted only in one way, ACB cannot be different from ABC, and, in particular, it must show the same ratio between the relative weights of its elements. Therefore, the ratio of the weights of A and C in forming the combination AC cannot be other than that expressed by the relative combining weights already found in the first way. In other words, it is possible to compute the composition of the hitherto unknown combination AC, from analyses of the combinations AB and ABC. In the same way, we can compute the composition of the unknown combination BC, by help of the numbers obtained by the analyses of two other combinations of the same elements. To resume: the combining weights relatively to A regulate all other possible compounds between the elements concerned. But this is nothing else than the general stoichiometrical law of combining weights, for we can extend our considerations without difficulty to any number of elements.

Lastly, it is easy to deduce the law of multiple proportions from the law of combining weights. If no compounds can be formed except according to their combining weights, then, if there are two different compounds between A and B, we can form the one containing more of B either directly from A and B, or indirectly, combining first A and B to form the lower compound and then combining this with more of B. In applying the law of combining weights, we conceive that the weight of B in the higher compound must be twice its weight in the lower. The same consideration may be repeated, and finally we get the result, that instead of double the combining weight, *any multiple* of it may occur in combinations, but no other ratio.

If we cast a backward glance on the mental operations we have performed in the last two deductions, we recognise the method, the application of which has made the two laws of energetics so fruitful. In the same manner as the difference between the whole and the available energy is

independent of the nature of the path between the same limiting points, the product of the chemical action between a number of given elements is independent of the way in which they are combined. If we compare two different ways, we get an equation between the characteristics of the two ways, and this is equivalent to a new law. In our case, this new law is the law of combining weights.

I will put the same idea into somewhat different words. By stating the equation between any two ways, we can get any number of different equations, each representing a new way as an experimental fact. Now, in order that all these equations shall be consistent, there must be some general law regulating the characteristics of the equations. For the consistency of the several equations in the case under discussion, the existence of specific combining weights, independent of the several combinations, is the necessary condition.

This is the main point of the considerations I wish to lay before you this evening. There are some secondary questions as to isomerides or allotropic states of substances, and there are other similar questions, but it would lead us too far to consider them one by one. I have investigated them on the same basis, and I can assure you that I have nowhere found an insurmountable difficulty or an impassable contradiction. All these facts find their proper place in the frame of the same general ideas.

Let me still add some words about the *nature of the elements*, as considered from my point of view. I wish to lay great stress on the fact that here, too, I find myself on the same ground as that on which Faraday built his general concepts during his whole scientific career. There is only one difference, due to the development of science. Faraday ever held up the idea that we know matter only by its forces, and that if we take the forces away, there will remain no inert carrier, but really nothing at all. As Faraday still clung to the atomic hypothesis, he was forced to express this idea by the conception that the atoms are only mathematical points whence the forces emerge, or where the directions of the several forces intersect; here his view coincided with that of Boscovich.

In the language of modern science I express these ideas by stating: *what we call matter is only a complex of energies which we find together in the same place*. We are still perfectly free, if we like, to suppose either that the energy fills the space homogeneously, or in a periodic or grained way; the latter assumption would be a substitute for the atomic hypothesis. The decision between these possibilities is a purely experimental question. Evidently there exist a great number of facts—and I count the chemical facts among them—which can be completely described by a homogeneous or non-periodic distribution of energy in space. Whether there exist facts which cannot be described without the periodic assumption, I dare not decide for want of knowledge; only I am bound to say that I know of none.

Taking this general point of view, in what light do we regard the question of the elements? We will find the answer, if we remember that the only difference between elements and compounds consists in the supposed impossibility of proving the so-called elements to be compounds. We are therefore led to ask for the general energetic properties underlying the concept of a chemical individual, whether element or compound.

The answer is most simple. The reason why it is possible to isolate a substance from a solution is that the available energy of the substance is a *minimum*, compared with that of all adjacent bodies. I will not develop this thesis at length, for it is a well known theorem in energetics or thermodynamics. I will only recall the fact that a minimum of vapour pressure is always accompanied by a minimum of available energy; and we have already seen that a minimum of vapour pressure or a maximum of boiling point is the characteristic of a hylotropic body or chemical individual.

This granted, we proceed to the question regarding the differences between the several substances. Expressed in the most general way, we find these differences connected with differences in their *specific energy content*. Temperature and pressure are not specific, for we can change them at will. Specific volume and specific entropy, on the contrary, are not changeable at will; every substance has its own



values of these. We may take therefore these values as the characteristics of the different substances. How many of such characteristics exist I cannot tell. Only for simplicity's sake I will assume that two of them are sufficient. As I will take care not to deduce any conclusions from this number, we shall not be led into error by accepting it.

We place these two characteristics in a system of planar coordinates; then the several elements will be represented by single points in the plane. We lay the plane horizontally and raise from these points ordinates, representing the available energy of each element. Between the points of the elements in the plane are situated the points of all possible solutions, filling up the whole plane. Each of these solutions will also have its available energy, and all the corresponding points in space will form a continuous surface. The form of this surface can be described in a general way. For as each element has its point in a *relative minimum*, the surface as a whole will have a shape like the ceiling of a cavern full of hanging stalactites, the end of each stalactite representing an element.

How can we pass from one element to another? Evidently not otherwise than by going over the higher parts of the surface, or the passes separating each stalactite from its neighbours. This can only be done by accumulating an appropriate amount of available energy in the element to be changed. Now the concentration of energy is a task we cannot accomplish *ad libitum*, for the possibility very soon ends. Think, for example, of compressing a gas into a given space. Up to some ten thousand atmospheres the work of compression will go on smoothly, but after that every metal begins to flow like a liquid, and you cannot proceed further. With the concentration of electric or any other energy the task is similar, and so we come to the conclusion that the concentration of energy can be pushed to only a very limited extent. The application of this result to our question about elements is simple enough: we cannot get over the pass between two stalactites because we cannot attain the necessary concentration of energy.

From the history of science we learn that these considerations contain at least some truth, for the isolation of the elements has ever been dependent upon the power of concentrating energy available at that time. The most brilliant example is the application of the voltaic pile to the isolation of the alkali metals by Humphry Davy.

Still I must confess that these last considerations are in a very embryonic state, and I should not have brought them before you if an unexpected application had not lately made itself manifest. Some years ago I explained these views to my old friend Sir William Ramsay, when he asked me how the idea of elements fitted into my conceptions of energy. Then I forgot all about it until Sir William reminded me of it, saying that his perplexing discovery of the transmutation of radium into helium might conceivably find some explanation in this way. This I am convinced of, and the considerations may be pictured in the following manner.

In the corner of our cavern where the elements with the highest combining weight are assembled, the stalactites are very short; and at last they are not really stalactites, but rather regions of different slope in the sloping ceiling. Where the plane is nearly horizontal a drop of water furnishes a picture of the stability of the elements. While hanging at the end of a true stalactite, more or less work must be done to raise the drop over the pass until it flows down another stalactite. But in this corner it will flow of its own accord, and only delay for a short time on the nearly horizontal portions in the ceiling.

Such elements will have only a *temporary existence*. Now we are sure that for the transmutation of one element into another enormous amounts of energy would be required, for the concentrations of energy as yet available have proved themselves insufficient for this purpose. We may expect, therefore, that enormous amounts of energy will be liberated if such an unstable element changes into a stable one. This accounts for the extraordinary quantity of energy developed by radium during its existence. The fact that radium changes into helium, an element with an exceptionally long stalactite (for it is impossible to get even any combination of helium), makes us expect indeed such

an unusually great development of energy as is found to occur.<sup>1</sup>

The heat from radium is surely only the last form of the energy developed in its transformation. There are a great many intermediate forms, termed rays or radiations, which have been studied by a band of eminent workers, whose ingenuity and ability have been displayed in the most brilliant way during these investigations. Perhaps I may venture the suggestion that first, other intermediate temporary elements are formed, and that the energy liberated at this transmutation appears first in the shape of *new*, still imperfectly known forms. It is most likely that such forms are originated during the decay of the enormously concentrated energy of radium; at the same time it is probable that we have not yet the means of fixing these forms and so preventing their changing into other more common forms. We should remember that, for example, the conservation of electric energy at a pressure of some thousand volts during some months or years is by no means an easy thing, and I have great doubt if it is possible at all.

But here I must conclude, for I have ventured to intrude on a field where I have not secured my own right of entry by personal work. I see among my audience men who are possessed of an incomparably more minute and comprehensive knowledge of these new realms of science than I. I must ask you, therefore, to take these suggestions in the same spirit as that in which Faraday took his own speculations. They are questions put to nature. If she says Yes, then we may follow the same path a little further. If she says No—well, then we must try another path.

#### A SMITHSONIAN MAGAZINE OF SCIENCE.

TO provide a medium for the early publication of the results of researches conducted under the auspices of the Smithsonian Institution, and especially for the publication of reports of a preliminary nature, a quarterly issue of the *Smithsonian Miscellaneous Collections* has been commenced. This new periodical has the form of an attractive magazine, and contains papers on a variety of subjects of scientific interest, most of them beautifully illustrated.

The number opens with a description of seventy new Malayan mammals, by Mr. Gerrit S. Miller, jun., based on collections made and presented to the U.S. National Museum by Dr. W. L. Abbott. Mr. C. G. Abbot presents the results of recent studies of the solar constant of radiation, conducted at the Astrophysical Observatory of the Smithsonian Institution, under the direction of Dr. S. P. Langley. Another paper by Mr. Abbot describes the new celostat and horizontal telescope of the Astrophysical Observatory, in which are given the results obtained with a device designed by Dr. Langley for the purpose of "churning" a column of air traversed by a solar beam, with the view of reducing the "boiling" or confusion of all parts of the solar image due to variability of the strata of air traversed. Dr. F. W. True presents some photographic illustrations of living finback whales from Newfoundland, these being the first photographs of living whales in American waters that have thus far been published. Brief descriptions of a skeleton of *Hesperornis*, and a new *Plesiosaur*, by Mr. Frederic A. Lucas, are given with plates, and Mr. W. H. Holmes illustrates and compares the designs on some remarkable shell ornaments from Kentucky and Mexico.

A noteworthy specimen of a Glacial pothole in the National Museum is described by Mr. George P. Merrill, who explains the method by which the specimen was procured. Some notes on the herons of the district of Columbia, by Mr. Paul Bartsch, who made a systematic survey of two heron colonies and conducted experiments with the view of solving some of the problems of bird life, are of special interest. Dr. J. Walter Fewkes gives a preliminary report on an archaeological trip to the West Indies

<sup>1</sup> Compare Soddy, "The Wilde Lecture," *Mem. and Proc. Manchester Lit. and Phil. Soc.*, 1904. I am very glad to find that I am in close agreement (except in so far as there is a difference in his accepting the atomistic, while I hold by the energetic point of view) with this most zealous and fortunate worker; indeed, the above statements were written and printed before I saw Mr. Soddy's lecture.